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STUDIES IN THE ORGANIZATION
OF MAN-MACHINE SYSTEMS

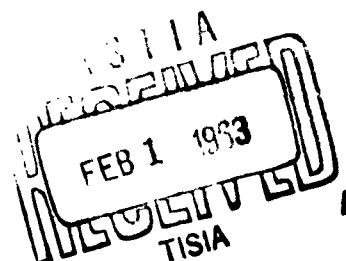
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C A M B R I D G E

N E W Y O R K

C H I C A G O

L O S A N G E L E S

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I. INTRODUCTION

The purposes of this report are to review briefly the general problem under investigation, the lines of attack followed during the course of the project, and the results and conclusions of the work. This is primarily an administrative report. References are made to published reports (Appendix) in order to keep the present report free of detail.

During the last twenty years, a vast amount of effort has gone into the design, development, construction, and operation of such military systems as the Semi-Automatic Ground Environment for air defense, the command and control systems used by the Strategic Air Command, and the launching, monitoring, and recovery systems associated with our missiles and satellites. The effort -- especially that part concerned with interactions between the human, equipment, and informational components of the systems -- has been very largely substantive and ad hoc to particular systems. Only a small fraction has concerned itself with general principles that might apply to several or all of the large military systems or with abstractions that might lead toward a science of system organization.

The work summarized is part of the small fraction. Its basic aim has been to find ways of increasing the transfer of knowledge among system programs and ways of relating research results to system applications. The methods used have not been the usual methods of research; they have been but informal mixtures of studying, discussing,

formulating, writing, and trying to understand and advance to some degree the technique of representing ideas and interrelations in the form of computer programs. The products are a few papers that describe parts of a picture that has taken shape during the course of the project.

II. THE GENERAL PROBLEM OF SYSTEM ORGANIZATION

In order to put into perspective the work to be described, it may be helpful to examine three facets of what, at the time the work started four years ago, was called "the systems problem". ("Systems" was usually set forth as plural, often with a capital "s", and "problem" was usually presented as singular.) The three facets were concerned with the questions:

- A. Why do research findings about man-machine interaction and system organization not find readier application in actual system development?
- B. Why do efforts to study large-scale systems in the laboratory repeatedly fail to produce satisfactory experimental results?
- C. Can we create a "science of systems"?

These questions were controversial and charged with considerable affect.

A. Lack of Application of Research Findings

At the outset of the work, most students of the systems problem knew or believed (1) that most of the established departments of science and technology, as well as several hybrid or "interdisciplinary" fields, were making contributions to the development of the large military systems; (2) that, however, application was running far behind research in some of the areas; and (3) that the discrepancy

was greatest in those areas that were the most abstract and most highly multi-disciplinary. There was widespread realization that the task of devising and constructing large military systems was at least an order of magnitude more complex than any of the tasks (except building and operating the telephone system) to which science and technology had hitherto been applied.

Despite that recognition, the fact that many principles and solutions (known to specialists in the various contributing disciplines) failed to be applied in actual system development led to frustration among some of the specialists, and - as frustration begets aggression - to the feeling that "management" was for some dark reason dedicated to the disregard of scientific and technical enlightenment except in instances in which (to use the then-prevailing jargon) a scientific or technical "breakthrough" promised a "quantum jump" in speed, attitude, or explosive force. It was recognized only by a few, and only vaguely, that the seeming disregard of proffered contributions might be understood in the system analysts' own terms as the natural and expectable behavior of a very large and complex organization. Among some of the people engaged in research on problems of system organization, therefore, morale was rather low. The systems problem seemed essentially to be one of bringing order and rationality into a vast, crucially important, but chaotic and irrational activity.

B. Failures of Early Efforts to Conduct Large-scale System Experiments

One approach to the study of system organization is to bring large-scale systems into the laboratory for formal, experimental study. Various techniques of simulation have been developed to facilitate this approach, a basic notion of which is that it is the field con-

text, rather than the size or complexity of systems, that makes it difficult to learn generalizable principles from actual operations. At the end of World War II, the NDRC Systems Research Laboratory at Beavertale Point, Rhode Island, was in the process of conducting laboratory experiments on naval radar and anti-aircraft systems. In the decade from 1947 to 1957, Project Cadillac at Port Washington, the System Coordination Facility at NRL, the Pi-Sigma Project at the Lincoln Laboratory, Project Cowboy at the Rand Corporation, the Man-Machine System Project at the Bureau of Standards, and the Isolated Crew Project at Lockheed, Marietta, all made serious attempts to conduct laboratory research on simulated man-machine systems of realistic size and complexity.

Although some of the efforts were very successful in other ways, none produced the kind of results we have come to expect, even in the "soft" sciences, from laboratory experiments. The visible difficulties were diverse: instability of early analogue computers, neglect of tangible problems in a context unfavorable for high abstraction, overwhelming acceptance (of practical implications) that diverted effort toward quick application, inadequacy of funding, etc. The question arose: Should one press on despite the repeated failures, or should he examine the problem of large-scale system experimentation to determine whether or not it is inherently feasible.

That question is of current interest and importance, for the climate seems now to favor new efforts to conduct laboratory experiments with large man-machine systems. The ESD-Mitre System Design Laboratory is expecting delivery of its Stretch computer next month; the System Development Corporation is readying its AN/APQ-32 machine;

and several industrial concerns have advanced plans for system experiments involving human subjects and computer simulation.

C. Science of Systems

Four years ago, there were quite a few workers who aspired to develop a new science. The "science of systems" was not to be merely a collection of system-applicable parts borrowed from established disciplines, but a science in its own right, dealing with principles and theorems that emerge when study is focused upon interaction itself and not just the interacting parts. (In the terms of the Gestalt dictum, "The whole [W] is greater than the sum [Σ] of its parts", the aspiration was to make a science of $W - \Sigma$.)

Among the scientists of established fields who came into contact with systems enthusiasts, the prevailing attitude was one of skepticism or perhaps disdain. There was no question that several of the sciences could contribute, directly or through their associated branches of technology, to system design and development. There was little disposition, however, to suppose that there would emerge from system-oriented research a distinctly new science; it was felt that the scientific outcome of such research would simply be more of what already existed in physics, chemistry, psychology, economics, etc. In the prevailing view, $W - \Sigma$ was either art of politics or zero.

III. LINES OF ATTACK

From the foregoing paragraphs, it may not be wholly clear just what the problem was, but it is surely evident that there was a problem. Since the basic aim of the work here reported was to advance the general understanding of systems, or at least to reduce to some extent the "ad-hoc-ness" of work on systems, it was possible to begin without defining the problem more precisely. The initial approach, therefore, was simply to explore about in various system contexts, looking for general or generalizable ideas. The line of attack became somewhat more definite as the work went on, and it changed from time to time in response to experience. Perhaps five different lines can be distinguished:

1. Search for general or generalizable principles;
2. Study of basic system tools;
3. Attempt to understand the process of system design;
4. Search for basic system concepts;
5. Attempts to understand and exploit the parallels between large-scale military man-machine systems and modern computer information-processing systems to find solutions to the problem of informational complexity seen in design, development, production, operation, and management of large-scale military man-machine systems.

Of these five lines of attack, only the fifth seems important in retrospect. It set the others into better perspective than they, themselves, provided. It shed a new light upon the three troublesome facets of the systems problem. And it gave rise to a fairly

definite (though not assuredly feasible) program for advancement of the system art in the direction of order and rationality.

The fifth line of attack, in short, appeared to be what we were seeking from the outset. There is no certain way, short of following it to its end, to prove that it is the proper line, and the field into which it leads is itself vast and largely unexplored. Nevertheless, with considerable conviction we can say that the key to system organization lies in that part of the information-processing field devoted to what we may call dynamic representation. In the remainder of this report, we shall try to elucidate that idea.

IV. RESULTS AND CONCLUSIONS

A. Heuristics and Algorithms

The first definite line of attack was a search, in the literature of man-machine systems, groups, and organizations, and experimental psychology, for system principles amenable to concise definition and wide application. This search was disappointing. Many statements were found that were called "principles" by their authors, but most of them that were not already established facts or laws of substantive sciences seemed lacking in operational meaning and lost substance when set into concise format. The remainder were specific to restricted contexts and resisted generalization. We therefore turned away from the search for generalizable principles and studied the problem of system-building tools.

By tools, in this context, we mean the conceptual instruments that aid men in devising systems: the theorems, theories, models, and prescribed procedures that extend man's intrinsic intelligence. These tools have been adapted into system applications mainly from the various branches of science and technology. Examples are statistical decision theory, game theory, servomechanism theory, linear and dynamic programming, and theory of linear networks. Each tool is the subject of specialized treatises, and most of the tools are brought together in such handbooks as Grabbe, Ramo, and Wooldridge's Handbook of Automation, Computation, and Control.

Looking back upon our search for principles and study of tools, we see a rough but useful analogy with heuristics and algorithms in the field of computation. Let us describe it briefly here but postpone detailed discussion until later.

The basic meaning of "heuristic" is "leading to insight or discovery". In the field of artificial intelligence, there is at present a trend toward use of the word as a very general rubric, subsuming all strategies, tactics, and procedures that lead - either invariably or usually - to advancement of understanding. In the present context, however, we need the older meaning, which opposes heuristic procedures to algorismic procedures. Heuristic procedures are intended or thought likely to lead to solutions or to new knowledge, whereas algorismic procedures are known, through definite proof, to achieve their proper goals.

In the sense of the distinction just made, most of the "systems principles" that have been described in the literature are roughly analogous to heuristics, and most of the system-building tools are analogous to algorisms.

If the whole task of devising a system to meet new requirements could be handled by what we are calling tools, there would be no need for analogues of heuristics. But new system requirements always raise new problems. The facts that new problems do arise, and that they can be solved only "through exercise of intuition and judgment" sets the system-building art into a very different class from, for example, the synthesis of linear networks. To the latter, a coherent body of definite theory applies. If one can state requirements succinctly, he can proceed almost by formula to an optimal design. Only when he comes to such questions as "electric vs. hydraulic" is "judgment" likely to enter, and even there a single decision procedure can be followed with confidence. To large military systems, many bodies of definite theory apply, but as fragments: they do not constitute an

over-all coherent body. (It is for that reason that they are regarded as tools, and not as an automatic factory.) In actual system development, the fragments are put into place and held there by judgment, intuition, rules of thumb, by what we found when we searched for principles. The art of system-building thus is an art of fitting algorithms and heuristics together into an over-all program.

The value of the analogy just suggested lies in the fact that, whereas no one knew in earlier decades how to represent clearly and definitely a complex consisting of such apparently incompatible parts, it is now fairly clear how to "mix" algorithms and heuristics. This is, we think, the great potential contribution of research in artificial intelligence to the field of systems. If the work here reported makes any contribution at all, we believe, it will lie in identifying the relevance and to some small degree developing the use of computer programs as system representations.

B. System Design

In the "Seminar on System Analysis, System Synthesis, and System Research" held during the first year of the project, the tutorial sessions were devoted to presentation of tools, but the discussion ran heavily to what we now recognize as heuristics. (The term "heuristic connection" was introduced into the Seminar discussion by Minsky, but the fundamental role of heuristics in system design

and development was not at once recognized.) On replay, the Seminar recordings were disappointing. We worked with them, and with transcripts, off and on for two years before figuring out what the trouble was. Then it slowly became apparent: as long as optimism was equated with "looking for a formula" or "trying to make a science", there was no chance to move forward in a realistic program. The circumstances fostered argument between optimists and pessimists and release of aggression by frustrated enthusiasts. The valuable parts of the Seminar - the tutorial presentations of tools and the occasional discussions of guide lines for judgment - were not properly related to one another because it was not perceived that the guide lines could be used as connective tissue to tie the diverse tools together into a coherent body of system-oriented knowledge.

The disappointment in the Seminar recordings and transcripts led us to examine the process of system design in an effort to discover how coherence is achieved when systems are actually devised. This examination revealed the importance of hierarchy in system design and it led us to see that coherence stemmed from what, in experimental psychology, is studied under the rubric, transfer of training. Effort to separate "hunches" from "tools" in transfer of training led us to see the relevance of the notion of heuristics.

C. Basic Concepts

For a period during which the fifth line of attack was formulating itself, we thought we had a fruitful approach in the examination

of such system fundamental concepts as system, organization, organism, subsystem, component, linearity, interaction, partitioning and synthesis from modules. The part of this work that seems significant in retrospect is that part concerned with the partitioning of systems into subsystems and the selection of variables and paradigms for experimentation. (Those problems are discussed in Report AFOSR-1127).

It is easy to show - simply by estimating the needed numbers of trials, tests, subjects, hours, etc. - that conventional multi-variable full-matrix experimental designs, of the type in conjunction with which analysis of variance is often used - are not feasible for research on complex man-machine systems. People continue to propose such research. Certainly they should be required to set forth reasonable estimates of what would be involved in carrying out the experiments - carrying them far enough forward to contribute something more than feasibility indications about preliminary tests designed to reveal possibly fruitful hypotheses. If the work reported here established such a requirement, and did nothing more, it would quickly save more than it cost.

It seems absolutely essential, on the basis of our study of the logistics of system experimentation, to develop experimental methods and designs for research on complex, multi-variable systems. Hill climbing seems to hold out some hope, but it has evident difficulties in "noisy" situations. The most promising guide line is to do most of the experimentation on models, checking the results of the model tests against a relatively few data from actual system tests.

From a practical point of view, the most important "basic concepts" are those related to synthesis from modules.

Almost all of man-machine research has been concerned with analytical experimentation in large-scale systems: (1) to partition each system into parts that interact simply and predictably, (2) to study the parts separately, (3) to develop models that will permit predictions of system behavior from knowledge of past behaviors, and (4) to spot-check the models against the actual system. Step (1) is extremely difficult. We do not know how to do it. How much easier and better - and more germane to system design and development - to synthesize from pre-tested modular parts! The main conclusion from our study of concepts is that man-machine-systems research should be reoriented away from analysts and toward modular experimentation and synthesis.

D. Dynamic Representation

At a point just past midway in the project, the significance of computer-program models of systems and system concepts became apparent. Almost all the effort of the last two years was devoted to investigation into computer-program representation of ideas and processes.

Much of the promise held out by computer-program representations is quite general. It stems from three characteristics: (1) Computer-program representation is intrinsically formal and complete; it is impossible to say something not interpretable within

the rules or to leave out a necessary detail and still have a running program. (2) Computer programs can represent anything that is expressible; they constitute a domain in which both heuristics and algorithms can be specified and in which heuristics and algorithms can interact. (3) When fed into an appropriate computer, computer programs become dynamic; they are transformed by the action of the machine from the static form to which other mathematical and formal-language models are wholly confined into a dynamic form that reveals their implications. Whereas the other main modes of representation are merely mnemonic and require human intervention if they are to develop, computer-program representations (i.e., computers and programs together) unfold of their own accord - they solve themselves.

Although computer-program representations have been with us for several years, and are well-known in the field of simulation, it seems not to be appreciated widely, even yet, what a completely revolutionary effect they can have on thinking and problem solving. As indicated, their advantages are quite general. However, they are so especially appropriate and essential to the representation of large, complex systems that they are sure eventually to dominate both the study and the design, development, production, and operation of such systems. It may be worthwhile, therefore, even at the risk of belaboring the obvious, to digress into an oversimplified but fundamentally accurate recounting of the development of thinking.

1. Very early man solved his problems overtly. If he wanted to move

a big boulder across a stream, he simply found a bridging tree trunk and pushed and hauled the boulder until it reached the other shore or rolled off into the water or broke the bridge. That direct approach to problem solving was slow and demanding of energy, but it left no room for perpetuated error.

2. Later men made pictorial representations to help them. The analogues they used were mainly spatial. They could draw sketches showing the local situation: boulder, stream, tree, and destination. They could even represent the tree-trunk as bent or breaking. It was much easier to draw pictures in the sand than to push big boulders. But the picture did not determine whether or not their bridges would collapse! In taking the burden off their muscles, they placed a responsibility upon their minds.

3. Pictorial representations were easy to connect with real world situations, but, as mentioned, difficult to transform from a priori to a posteriori state. To avoid the repeated redrawing of pictures, still later men abstracted further and developed symbols. (Hieroglyphs were part of the transition.) They learned how to devise calculi, and thus increased their capability to progress symbolically from initial to subsequent conditions - i.e., to predict. Although the cost in cerebration was great, and although errors precluded truly deep calculations, men used essentially this method of solving their engineering problems through World War II. The essence of the method was to employ representations that were easy to manipulate, and literally to manipulate the representations - thus to rely on being smart instead of relying on being strong.

4. Now, dynamic representation makes it possible to succeed without being either strong or smart. The symbolic representations are turned into a computer program. The computer performs the manipulations of the symbols with great accuracy and inexorable logic. Very much deeper problems, very much more complex problems can be solved - faster, more accurately, at far less cost.

What remains for man to do? Two things: (1) Actually to exploit this great advance in the art of thinking, which is as yet scarcely exploited at all in formulative thinking. (2) To master more fully the initial and final steps of the procedure. The initial step is to abstract from the real world and to link real-world parts with model parts. The final step is to translate back from the model to the real world. These steps involve perception perhaps more than thinking. They are not made easier by the introduction of the dynamic quality.

IN SUMMARY, then, the main outcomes of the study of system organization were:

1. Recognition of a close analogy between large-scale military man-machine systems and systems of computer programs,
2. Recognition of the representational capabilities of computer programs, and
3. A set of ideas about how to exploit the analogy and the capabilities in study of systems and in substantive work on systems. The latter ideas are set forth in Reports AFOSR 1127 and AFOSR 1673.

JCRL/cm

V. APPENDIX

The following reports were prepared under the contract:

J. C. R. Licklider, "Man-Computer Symbiosis," IRE Transactions on Human Factors in Electronics, Vol. HFE-1, No. 1 (March, 1960), AFOSR-TN-60-1191.

J. C. R. Licklider, "Bridges over the Gulf between Man-Machine-System Research and Man-Machine-System Development," chapter for a book to be published by Munksgaard 6, Nørregade, Copenhagen K, Denmark. AFOSR 1127.

J. C. R. Licklider, "The System System," Human Factors in Technology, (Edward Bennett, James Degan, and Joseph Spiegel, eds.) New York: McGraw-Hill Book Co., Inc., 1962. AFOSR 1673.

J. C. R. Licklider, "On Psychophysiological Models," Sensory Communication (W. Rosenblith, ed.), New York: John Wiley and Sons, 1961. AFOSR-TN-60-1190.

J. C. R. Licklider, "Three Auditory Theories," Psychology: A Study of a Science, 1, (S. Koch, ed.) New York: McGraw-Hill Book Co., Inc., 1959.

J. C. R. Licklider, "Periodicity Pitch and Related Auditory Process Models," International Audiology, Vol. 1, No. 1, 1962. AFOSR 2681.

J. C. R. Licklider, "Interactions between Artificial Intelligence, Military Intelligence, and Command and Control," Proc. First Congress on the Information Sciences, Hot Springs, Va., Nov., 1962

The following talks were made on project topics:

J. C. R. Licklider, "Man-Computer Communication," Symposium on Digital Computing in a University, Massachusetts Institute of Technology, Cambridge, Mass., January, 1962.

J. C. R. Licklider, "Man-Computer Symbiosis," Colloquium of the Advanced Research Projects Agency, Washington, D.C., June 8, 1962.

J. C. R. Licklider, "Man-Computer Communication," Presidential Address, Society of Engineering Psychologists, St. Louis, Mo., September 3, 1962.

J. C. R. Licklider, "Man-Computer Communication," New England Psychological Association, Brandeis University, Waltham, Mass., March, 1962.

J. C. R. Licklider, "Computer Programming as a Communication Technique," Computation Center Colloquium, Massachusetts Institute of Technology, Cambridge, Mass., April, 1962.